

Riserratevi sotto coverta ...[☆]

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Abstract

The principle of relativity, as originally expressed by Galileo, points out that the area of competence of the principle itself is that of isolated systems as well as inertial reference frames. The principle does not claim that it is always possible to isolate any physical system, indeed it leaves it open to the possibility of the existence of phenomena concerning non isolable physical systems, e.g. phenomena regulated by some non draggable ether. After the Aspect experiment realist and local models have been proposed specifically based on the hypothesis that entangled systems are not isolated. It is hypothesized that the correlations which allow the violation of Bell's inequality are due to exchanges of superluminal signals between the various parts of the system and those signals do not generate causal paradoxes because their propagation is regulated by a non draggable ether. In the present paper the perfect compatibility of such models with the relativity theory is strongly advocated. A criterion is finally proposed to determine the causal ordering between events since, when there are superluminal signals, that ordering can no longer be associated to the time ordering induced by the standard synchronization.

Keywords: Principle of relativity, EPR experiments, Causality, Superluminal signals

1. Introduction

Galileo, "Dialogue concerning the two chief world system" [1] (pages 216-217), writes the following passage, which has become famous as the first statement of the principle of relativity.

Shut yourself up with some friend in the main cabin below decks on some large ship, and have with you there some flies, butterflies, and other small flying animals. Have a large bowl of water with some fish on it; hang up a bottle that empties drop by drop into a narrow-mouthed vessel beneath it. With the ship standing still, observe carefully how the little animals fly with equal speed to all sides of the cabin. The fish swim indifferently in all direction; the drops fall into the vessel beneath; [...] When you have observed all these things carefully (though there is no doubt that when the ship is standing still

everything must happen in this way), have the ship proceed with any speed you like, so long as the motion is uniform and not fluctuating this way and that. You will discover not the least change in all the effects named, nor could you tell from any of them whether the ship was moving or standing still.

Over the years the principle of relativity has been presented in different ways by different scientists. The focus of our work is the importance to be "below decks". This point is adequately stressed by Galileo and maybe implied by modern authors as well; in my opinion, however, it is implicitly overlooked in order to achieve some results modern scientists almost unanimously agree on. In the "demonstrations" of the impossibility of superluminal signals (also called tachyons) almost all modern authors pass over the condition of being "below decks" and these demonstrations are then given, more or less unconsciously, a validity which goes beyond the hypothesis on which they themselves are based. The consequence is often a sharp refusal to take into account any consideration which hypothesizes the ex-

[☆]Shut yourself up below decks ...

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istence of superluminal signals since it is generally believed that it is proved that those signals give rise to paradoxes, or that their existence undermines the theory of relativity. Even when confronted with astonishing experimental results, like those obtained in the various experiments on the violation of Bell's inequality, e.g. the Aspect test, only a few scientists have agreed with Bell¹ [2], stating that "in these EPR experiments there is the suggestion that behind the scenes something is going faster than light" (page 49). The prevailing opinion has been, and still is, that the Aspect test marks the end of the local realism. This is the amazing conclusion on which there is almost unanimous agreement since it is believed that the facts themselves (the Aspect test) compel us to accept it. The main purpose of these pages will precisely be to demonstrate that it is not compulsory and not even necessarily preferable from the epistemological point of view, to interpret the outcome of the Aspect test in the currently dominant way. If we conveniently emphasize the condition of being "below decks", which must be expressed in the principle of relativity, it can be easily shown that the superluminal signals could exist without generating any paradox or arising any problems for the theory of relativity; the Aspect test might also find an interpretation which, depending on personal ideas (or the assumed epistemological view), could be considered far less amazing than the end of local realism.

2. Reference frames and systems of coordinates.

Before analysing the extract by Galileo quoted above, we must clearly define what will be meant by "reference frame" in the following pages, specifying the clear distinction with the concept of "system of coordinates".

We will adopt the definition given by E.Fabri [3] (page 40):

But what is a reference frame? With reference frame I mean an environment, a laboratory, a *real physical object*, material, really existing, identified in practical terms. Incidentally, a reference frame

must be rigid: in a lab deforming before my eyes, built like an accordion, it is difficult to carry out all the measurements or to interpret them. I will also imply that this laboratory-environment is equipped with all the measurement instruments I need. [...] You should think that in a reference frame, identified as a laboratory, a room, there are all the instruments you need to take measurements, to carry out an experiment. This is a reference frame.

Therefore, when I mention two different reference frames you should think of two such environments separated: that's all. Naturally, nothing prevents me, if, for instance, our reference frame is this classroom, from agreeing that x , y and z are respectively the Cartesian coordinates to identify the various points. However, instead of assuming that SC [system of coordinates] I can take any other, without changing the physics for that; only the numbers expressing the coordinates will change.

Briefly, the reference frame, as defined by E. Fabri, is Galileo's "main cabin on some large ship". And as in Galileo's main cabin of the large ship there are flies, butterflies, a large bowl of water, some fish, a small bucket, in the reference frame defined by Fabri there are "all the instruments" necessary to carry out any experiment. Once a reference frame within which a certain experiment is carried out has been fixed, any system of coordinates can be chosen freely (for instance, Cartesian, or spherical, cylindrical or others) to describe the experiment itself. The relativity principle obviously concerns the reference frames, not the systems of coordinates. Precisely it concerns the *inertial* reference frames. We should specify which feature makes a reference frame (a room, a lab) inertial. Galileo points out also that the relativity principle concerns reference frames "below decks", we should therefore also point out which condition should be satisfied in order to state that a reference frame is "below decks".

3. The relativity principle

Let's now analyse the passage by Galileo in order to identify the "gist" of the principle he states.

Let us focus on what is with a modern expression called *inertiality* of the reference frame. Galileo

¹The book "The Ghost in the Atom" [2] is a collection of a series of radio interviews to various physicists from BBC Radio 3. The passage quoted is taken from one of the answers given by Bell to the interviewers.

defines it assuming that the motion of the ship “is uniform and not fluctuating this way and that”. In order not to fluctuate this way and that, the ship must not have interactions with the external world, or, at least, its “main cabin”, which forms our reference frame. We can also say that the sum of all the interactions the reference frame (the cabin) has with the external world must be null. For instance, the ship might interact with the Earth, and, because of that interaction the ship would be subject to the gravity force, it might then interact with the sea water which exercises on the ship a force opposite to the gravity force. Naturally, if we wanted to define as inertial a reference frame having null interaction with the external world, we might have the problem to define the concept of “interaction”, or, even better, the concept of “absence of interaction”; in my opinion, however, such a concept should be considered as primitive. I do not deny the importance of discussing about the correct definition of the concept “absence of interaction”, but I maintain that this topic goes well beyond the area of competence we are setting ourselves. Whichever the correct definition of that concept is, the fact is that the relativity principle concerns inertial reference frames and that, at least in my opinion, the definition of inertial reference frame is impossible without using the concept of “absence of interactions”. Even if we wanted to define as inertial a reference frame where the first principle of dynamics applies, we would find ourselves in a situation in which the concept of absence of interaction is considered as primitive.

Let us now turn to analyse the other point Galileo makes: the inertial reference frame must be “below decks”. Let’s hypothesize we are carrying out a certain experiment in our inertial reference frame. In such an experiment some measurement instruments as well as some objects which form the system under experimental study will be involved. I think that the correct interpretation of the concept “below decks” should be that the external world must not interact with the system under experimental study. In other words, the system must be *isolated*. Naturally, during the experiment, our system will be subject to some interactions; what we hypothesize, however, is that such interactions find all their causes within our reference frame.

We have therefore an inertial reference frame which, as such, does not interact with the external world and, consequently, all the measurement instruments solidal to the reference frame will not

receive from the walls any interaction from the external world. The experiment we are carrying out inside our inertial reference frame will concern a system which we hypothesize it is isolated. All interactions occurring during our experiment will consequently be *internal* to the reference frame, that is, they will be interactions between parts of the system or between these and some measurement instrument.

The relativity principle claims that repeating the same experiment within two different inertial reference frames the same results will be obtained if the system under our experimental study is *isolated*.

In fact a more extensive form can be given to the relativity principle, which does not limit it to necessarily isolated systems. Galileo knows very well that, inside his boat, a privileged direction exists, called “vertical”. It is the direction of the motion of the bodies which are dropped from a still condition. When he says that the outcome of two experiments carried out on two different boats, having a different state of motion with respect to the sea, will always be the same, he is assuming that the two boats are placed in the same way with respect to the vertical. We know that there are various experiments to enable us to understand how the boat is placed with respect to the vertical. For instance, it is sufficient to place a small ball on a table, if it does not roll it means that the surface of the table is perpendicular to the vertical direction.

Galileo does *not* claim the physical equivalence of all inertial reference frames in relation to any one test. Besides, he does not claim that an “outside” with decisive effects on our experiments (or at least on some of them) cannot exist. Two boats turned 10 degrees one with the other around an axis perpendicular to the vertical are surely not equivalent: in one the ball stays still on the table, in the other it does not.

Actually it is not true that Galileo’s reference frame is shielded from any possible external influence. Newton revealed that the privileged direction is given by the interaction of the bodies with the Earth (i.e. this direction is established by the relation between our boat and the Earth), however, following what Newton says, Galileo’s principle could become:

the same experiment must be carried out in two different inertial reference frames trying to shield them at its best from the effects that the outside has on the experimental apparatus. Should resid-

ual interactions due to the outside exist, having decisive effects on the experimental apparatus, all efforts must be made to ensure that the effects are the same in both reference frames. The principle claims that only after all these precautions have been taken, will the experiments carried out in the two reference frames will have the same outcome. Naturally there is the problem of finding a criterion to understand whether the outside has decisive effects on the experimental apparatus or not. The relativity principle indicates such a criterion: the same *operations* are repeated in two different reference frames, both the operations to prepare the system under experimental study and the measurement operations; if the measurements have different results in the two reference frames we will say² that the outside has decisive effects on the experimental apparatus, i.e. it has decisive effects either on the system (at least one of the two reference frames is not below decks, i.e. it is not isolated) or on the measurement instruments (at least one of the two reference frames is “fluctuating this way and that”).

When we say that the outside has decisive effects on our experimental apparatus we do not mean non-local interactions. Any experiment always consists in the preparation of a certain system in a certain way (preparation of causes), and in the observation of some results of some measurements made on the system (observation of effects). We say that the system object of our experimental study is “below decks” if the evolution of our system depends exclusively on the causes that have been prepared by the experimenter. The concept of “below decks” does not consist in the mere assertion of the locality of the interactions (which is implied). The concept defines systems which are composed only of subsystems that the experimenter has prepared purposely in the inertial reference frame in which the experiment takes place. A system is not below decks if its parts may interact locally, and then internally to the reference frame, with some subsystem that has not been prepared by the experimenter like, for example, a field generated by entities that are located “above decks” which has not been properly

²The use of this verb shows the epistemological value of the principle under study (as of all the principles generally employed in physics): we do not claim to be making a *true* statement, we only state a *modus operandi*. Fundamentally we say “Let us try to interpret the events following this principle”.

screened.

This is the origin of the possible non-invariance of the measurements performed on systems that are not below decks.

But this possible non invariance does not affect in any way the principle of relativity which states the invariance of the measurements only for systems that are below decks.

The topic (i.e. the identification of the gist of the relativity principle) could be presented in other terms too. The basic assumption is that the phenomena, at least those physics deals with, have some causes. The relativity principle claims that the repetition of the *same* causes will result in the same effects, regardless of *where* and *when* the repetition of the causes occurs, that is regardless of the reference frame inside which the experiment is carried out.

For instance, cutting the string of given length which is compressing a spring of given force constant and given rest length on which the body C of given mass is placed at one end and the body C' of given mass at the other (preparation of the causes), it can be observed that the measurement of the momentum of the body C will have a certain value p in the moment in which C moves away from the spring (effect). It has no relevance which is the reference frame in which string, spring, C and C' have been placed (the reference frame will necessarily have to be inertial if the measurement instrument is solidal to the reference frame, as it usually is). What matters is only that the preparation of the causes is carried out exactly in that way; for instance it is important that, before cutting the string, there is no relative motion between string, spring, C , C' and the instrument used to measure the momentum of C placed where C will move away from the spring³. As that effect (the

³A momentum measurement instrument of body C could be a rule, long L , with the direction of the motion of the body (that is the direction of the spring), equipped with a recorder of instants τ_{in} and τ_{fin} , shown on the clock in motion with C , in the moments when the clock (and C , too) goes by the two ends of the rule. We will have $p = m \frac{L}{\tau_{fin} - \tau_{in}}$, where m is the mass of the body C . The correct use of the instrument requires the instrument to be in rest with respect to the spring, the string and the body C before cutting the string and not to be subjected to interactions at least until the measurement procedure has been completed (the only admitted interactions are those strictly connected to the measurement to take, that is those which allow the recordings of the instants τ_{in} and τ_{fin}).

measurement of the value p) has only those causes (string, spring, C , C' and measurement instrument prepared in that way), the rest of the universe has no importance in order to observe that effect. Particularly, the state of motion of the rest of the world with respect to the system under study has no importance whatsoever.

Probably it will be always impossible to be aware of *all* the causes of a certain effect. The modus operandi we assume (the relativity principle) tells us that repeating all the causes we know we should observe the same effect, it also indicates the possible reasons which could determine our failure to observe the same effect:

a) a certain cause we are aware of has not been repeated correctly;

b) there is a further cause, inside the reference frame in which the experiment is carried out, which we do not know;

c) the reference frame is “fluctuating this way and that”, i.e. it is in relation with the outside. This interaction, passed on to the measurement instruments which, as prescribed in the instructions for the preparation of the experiment, are fixed to the reference frame (or in a precise state of motion relative to the reference frame), makes the outcomes of the measurements different from those we would have if the reference frame were not “fluctuating this way and that”;

d) the reference frame is not “below decks”, that means our system has relations with the outside we are not aware of, consequently, we have neither been able to shield the system from those interactions nor to prepare it so that such interactions would repeat always in the same way.

The points a) and b) are obvious (so obvious that the relativity principle does not mention them; however, we could imagine they are implied). The former simply states that the experiment has been carried out incorrectly, the latter that the phenomenon under study is not perfectly known yet⁴. For instance, in the experiment above, using a sufficiently refined instrument, we might observe a different p from one day to the next, and then realize that among the possible causes there is the air humidity rate inside the reference frame.

⁴This is essentially always true. The point b) is the one we always turn to, at least at first, whenever a certain experiment, repeated after improving the experimental apparatus, produces results different from those expected.

The point c) is well known and states that an experiment prepared in a non inertial reference frame might have different outcomes from those obtained from an experiment prepared in an inertial reference frame.

The point d) is the one we are particularly focusing our attention on. Actually, we are focusing it on the fact that the relativity principle predicts that *there is also* the point d), that is, it foresees that there might be different outcomes when repeating the experiment in two different reference frames even if it were true that:

- all the causes we know have been repeated in a perfectly identical way (point a);

- we can show that no cause we are not aware of can exist inside the two reference frames (point b);

- the two reference frames are inertial, as confirmed by the fact that all the experiments carried out before have always had the same outcomes in the two reference frames (point c).

Galileo claims that, in a situation like the one presented above, we must not question the validity of the relativity principle yet, we must not doubt yet that the repetition of the same causes produces the same effects, on the contrary we must consider the hypothesis that at least one of the two reference frames may not be “below decks”, that is the outside interacts with our system and the interaction is different in the two reference frames.

Going back to the experiment in our example, we could realize that rubbing the body C two different results are obtained in the two different inertial reference frames. It might happen that, with the passing time, in one reference frame the difference between p measured rubbing C and p measured without rubbing it will increase, while in another reference frame such a difference will decrease. Since the two reference frames are inertial (they move with “free motion”, i.e. the rooms where the experiments are carried out, the two laboratories, do not interact with the outside; and neither do the measurement instruments within the labs) and the procedure followed for the preparation of the experiments in the two laboratories is the same, the relativity principle require us to go “out there” to look for the causes which might produce different results. Some confirmations may be found out there. For instance, an electrically charged body may be found, to which one reference frame is drawing up while the other is moving away.

We conclude this paragraph stressing again our

thesis.

In many texts it is said that, according to the relativity principle, it is not possible to identify any experiment which may produce different outcomes in two different inertial reference frames. For instance in 1921, in “The meaning of relativity”[4] Einstein claims (page 14)

If K is an inertial system, then every other inertial system K' which moves uniformly and without rotation relatively to K , is also an inertial system; the laws of nature are in concordance for all inertial systems. This statement we shall call “principle of special relativity.”

Such a statement is too strong and does not respect the spirit of the principle originally claimed by Galileo. At least in the opinion of the writer of these pages. Einstein’s expression “moves uniformly and without rotation” is Galileo’s “the motion is uniform and not fluctuating this way or that”, in Einstein’s sentence, however, Galileo’s “below decks” is missing.

We can also note that, in the famous article of 1905, Einstein[5] says more cautiously:

Examples of this sort, together with the unsuccessful attempts to discover any motion of the earth relatively to “light medium” suggest that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest. They suggest rather that, as has already been shown to the first order of small quantities, the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good. We will raise this conjecture (the purport of which will hereafter be called the “Principle of Relativity”) to the status of postulate [...]

In the 1905 statement Einstein does not say that *all* natural laws “will be valid for all frames of reference for which the equations of mechanics hold good”. He confines himself to mechanics and electrodynamics, he will then refer to rules and clocks (saying basically that rules and clocks behave in the same way in every inertial reference frame). He will build up the relativity on the basis of these minimal hypothesis. This does not mean that relativity should be limited to mechanics and electrodynamics. Every phenomenon which finds its

causes within the reference frame (excluding consequently phenomena ruled by an “ether” in which the reference frame is immersed) will find a similar description for every reference frame. However, a phenomenon must not necessarily find all its causes within the reference frame. A phenomenon must not necessarily be described in the same way in every reference frame.

Definitely, resuming the experiment in our example, the “law of nature” which regulates the motion of the body C rubbed is different in the two reference frames. It becomes the same only if the two reference frames are shielded from the effects of external charges, namely if the two reference frames are placed “below decks” also in relation to experiments carried out with bodies electrically charged. Particularly relevant is the fact that the possible discovery of a phenomenon which we can not shield from the interactions with the outside (with the consequence that the “law of nature” for that phenomenon may be different in the different reference frames) *does not create any problems* for the relativity principle which indeed predicts that such phenomena may exist and also explains how to act in such a circumstance (the causes must be looked for “out there”).

Even if we did not know how to shield a reference frame from external electrical fields and were consequently forced to describe the electrical phenomena differently in each reference frame (as different reference frames are hit by external electrical fields in theory different one from the other), still the fact that the physical laws of the electrically discharged bodies are the same in all inertial reference frames would apply. The possible finding of a phenomenon for which external causes to the reference frame were decisive (e.g. a phenomenon with a preferred reference frame in which the outcomes of the measures associated to the phenomenon, or at least some of them, are different from the outcomes of the same measures carried out in other reference frames) *would not change in any way* the laws of physics which describe the phenomena with causes within the reference frame.

The possible discovery of phenomena with a preferred reference frame would not create any problems for the relativity.

In the following pages we will call *inertial* a reference frame (a room, a lab.) which does not interact with the outside, that is a reference frame in which the measurement instruments do not have an interaction with the outside, not even indirectly

through their interaction with the walls of the reference frame which consequently must not themselves interact with the outside.

We will call the following statement the *relativity principle (PR)*:

repeating a certain experiment on a certain system within two inertial reference frames, the same results will be obtained in both reference frames if the system under experimental study does not interact with the outside. The same outcome will be achieved even if the outside interacted with the system or with the reference frames only if such interactions are made the same in both reference frames. We will call the following statement the *relativity principle in its strong form (PRF)*:

repeating a certain experiment on a certain system within two inertial reference frames the same outcome will *always* be obtained in both reference frames. In order to obtain the same outcome it may be necessary either to shield the reference frames or to repeat the possible interactions with the outside of the system under experimental study in the same way in both the reference frames. It is assumed that at least one of the two possibilities (the shielding or the identical repetition in the different reference frames) will always be accessible by means of operations that could be performed within the reference frame.

The difference between *PR* and *PRF* is clear: the former admits the possibility that there may be phenomena for which it may not be possible to be placed “below decks”. The *PR* lets nature decide whether, for a certain phenomenon, it is possible to be placed below decks (that is it is possible to work so that the specific phenomenon could have all its causes within the reference frame, alternatively the possible residual external causes be repeated in the same way in every reference frame). The *PRF* states a prescription with which nature should comply: for any phenomenon it must always be possible either to shield it from the interactions with the outside which might affect the phenomenon or to repeat those interactions exactly in the same way in every inertial reference frame (that means either that the phenomenon, possibly after the shielding, will find all its causes within the reference frame, or that the possible external causes from which it can not be shielded could always be repeated in the same way in every reference frame).

4. Tachyons and causal paradoxes

The theorem showing the relation between tachyons and causal paradoxes is discussed in several texts: e.g. Møller (1952) [6] pages 52-53, Bohm (1996) [7] pages 186-189, Regge (1981) [8] page 21, Penrose (1989) [9] pages 274-275, Rindler (2006) [10] page 55.

As for all theorems, of course, there is a thesis and there are some hypothesis on which we base ourselves. The thesis is exactly the paradoxicality of the tachyons which would allow us to communicate with our past. For instance, using tachyons, in the afternoon, in the middle of a storm, we could warn ourselves in the morning to take the umbrella as it would start to rain around midday. The paradox is that, being soaked, we could act as to avoid getting wet.

We will see that not all the hypothesis of the theorem will be necessarily proved and that it will be possible to deny the thesis (claiming that the existence of tachyons is not necessarily paradoxical) leaving all the physics known so far basically unaltered.

In less recent works the theorem is presented incompletely: Einstein (1907) [11] pages 379-382, Tolman (1917) [12] pages 54-55, Pauli (1921) [13] page 34, Von Laue (1922) [14] page 70. These “demonstrations” are based on the fact that a tachyon sent from one point to another in a certain reference frame, if observed from a reference frame R' having adequate motion in relation to R , would prove leaving from a certain point at the instant t'_{in} and arriving at another definite point at the moment $t'_{fin} < t'_{in}$. This fact is considered paradoxical, but one forgets that the instants t'_{in} and t'_{fin} (shown on *different* clocks), as all the other instants shown on any clock in the reference frame under study, depend decisively on the synchronization procedure conventionally adopted. Consequently it appears obvious that, on the base of conventional assumptions, no physically meaningful conclusion can be drawn.

The demonstration can be easily completed (as it is done in many recent works [7–10]) assuming that we can send from R' to R tachyons having the same characteristics of those sent from R to R' . This assumption is considered valid on the base of the relativity principle expressed in the form called above *PRF*, not in the one originally expressed by Galileo [1] (*PR*) which, as already said, is consistent with the principle stated in the form chosen by Einstein

[5] in 1905.

Møller [6] represents an exception: he bases his demonstration on less strong hypothesis than those in [7–10]. Since the theorems expressed in [7–10] can be obtained as particular case in the demonstration given by Møller, we will discuss only this last in detail. It is just thanks to the extent of the hypothesis assumed that Møller's demonstration will, at first sight, seem unchallengeable. We will see that this is not the case.

4.1. Møller's demonstration

Møller considers two reference frames, S and S' , in uniform relative motion, where coordinate axis have been defined (x, y, z) and (x', y', z') in the usual way, and where the clocks have been synchronized standardly. The motion of S' with respect to S is defined by the speed $(v, 0, 0)$. Both the clocks fixed at the origin of the two reference frames show the instant 0 when the two origins are overlapped. The link between the coordinates defined in the two reference frames is then given by Lorentz's transformations in their usual form:

$$\begin{cases} x = \gamma(x' + vt') \\ t = \gamma(t' + vx'/c^2) \\ y = y' \\ z = z' \end{cases}$$

where $\gamma = (1 - v^2/c^2)^{-\frac{1}{2}}$ with $-c < v < c$, being v the speed of S' with respect to S .

Møller hypothesizes that, in the moment when the origins of the two reference frames overlap, a tachyon with speed $u' > c$ in S' can be sent out, from the origin, in the negative direction of the axis x' . It should be noted that, no hypothesis is made about the motion of the emitter of the tachyon. We do not hypothesize that the emitter is fixed in S' or in S . We only hypothesize that, somehow, this tachyon has been sent out from that point towards that direction. And we simply say that, if the signal sent out is a tachyon, it will be seen like that in S' . The fact that, if a signal proves superluminal in a reference frame, then it will prove superluminal in every reference frame, comes from the invariance of the difference $dt^2 - dx^2/c^2$: if in one reference frame $dt^2 - dx^2/c^2 < 0 \Leftrightarrow (dx/dt)^2 > c^2$, then in every reference frame it will be $u'^2 = (dx'/dt')^2 > c^2$. This last equation is considered equivalent to $u' > c$ since the sign of u' is thought to be given by the direction of propagation of the tachyon. Basically we say that, if, when the clock fixed at the origin

shows the instant 0, it is possible to send out a tachyon from the origin in the negative direction of the x' (and we hypothesize it is possible) then the equation describing the motion of that tachyon, in the reference frame S' , will be $x' = -u't'$ with $u' > c$ (positive).

At this point Møller says that the tachyon will arrive in the point P , associated to the abscissa $x'_p < 0$ in the x' axis, in the moment when the clock fixed in x'_p shows the instant $t'_1 > 0$, with $x'_p = -u't'_1$. So Lorentz's transformations give the space time coordinates associated with the event "arrival of the tachyon in P " in the reference frame S (eqs. (50) in [6]):

$$\begin{cases} x_p = \gamma(x'_p + vt'_1) = -\gamma t'_1(u' - v) < 0 \\ t_1 = \gamma(t'_1 + vx'_p/c^2) = \gamma t'_1(1 - vu'/c^2) \end{cases} \quad (1)$$

We now hypothesize that, soon after the arrival of the tachyon in P , a new tachyon is sent out from P towards O and that the speed of this latter tachyon is $w > c$ with respect to S . As before, we could say that for this second tachyon too only the bare essential has been assumed: the reference frame S must see this second signal as superluminal. Thus, as the first hypothesis seemed summarizable as "tachyons can be sent out in the negative direction of the axis x' " this seems summarizable as "tachyons can be sent out in the positive direction of the axis x ".

If we laid down $w = u'$ justifying the position on the account of the *PRF* (if there are tachyons which S' sees moving at the speed u' towards S , then there must be tachyons which S sees moving at the speed $w = u'$ towards S') then the demonstration would become equivalent to the one given in [7–10], but Møller does not fix $w = u'$, thus making his demonstration more general. Møller's hypothesis would simply appear that, given any direction, it is possible to send out tachyons along that direction, in both ways.

The demonstration goes on by clarifying the equation of motion of the second tachyon in the variables x, t , defined in S (eq. (51) in [6]):

$$x = w(t - t_1) + x_p.$$

From this equation, employing (1), we will obtain that the second tachyon will reach O ($x=0$) when the clock fixed in O shows the instant t_2 given by (eq. (52) in [6]):

$$t_2 = t_1 - x_p/w = \gamma t'_1 \left(1 - \frac{u'v}{c^2} + \frac{u' - v}{w} \right).$$

We then have that the first tachyon leaves O when the clock fixed in O shows the instant 0 and the second tachyon (whose departure from P , we could imagine, has itself been caused from the detection in P of the first tachyon) reaches O when the clock fixed in O shows the instant t_2 . If it could be $t_2 < 0$ we would have a clear causal paradox.

Altering slightly the last step of the demonstration given by Møller⁵, we notice that

$$v > \frac{u' + w}{1 + \frac{u'w}{c^2}} \Leftrightarrow \left(1 - \frac{u'v}{c^2} + \frac{u' - v}{w}\right) < 0 \quad (2)$$

and being $t'_1 > 0$ we have that if v , the speed of S' with respect to S , were higher than $(u' + w)/(1 + u'w/c^2)$, we would then have the paradoxical thesis $t_2 = \gamma t'_1 (1 - u'v/c^2 + (u' - v)/w) < 0$. As for every $u' > c$ and $w > c$ we always have $(u' + w)/(1 + u'w/c^2) < c$, the paradoxical thesis would appear inevitable, subject to adequate choice of v .

4.1.1. Analysis of Møller's demonstration

Between the lines of Møller's demonstration there is a hypothesis which could be missed out if not stressed properly. Saying that it will always be possible to choose v higher than $(u' + w)/(1 + u'w/c^2)$ the fact that the rise of v may be uninfluential on the values of u' and w is taken for granted. Actually, we take for granted that, if u' and w depended on v , this dependence would be such to make it anyway possible to choose an adequate $v \in (-c, c)$ so to satisfy the (2). This hypothesis has physical significance, it describes the behaviour of nature. Naturally, it is correct to assume hypothesis of physical significance and to find their consequences, but it is important to be well aware of that. If we wanted to deny the consequences, we would know which physical hypothesis we should reject. In our specific case, if we wanted to deny the thesis of the paradoxicality of the superluminal signals, we should prove the existence of alternative physical hypothesis on the basis of which the (2) proves always unsatisfied. A possible alternative hypothesis is the existence of a preferred reference frame, S_0 , which supports the propagation of the tachyons, as the air supports the

propagation of sound signals. Once the clocks in S_0 have been synchronized according to the standard relation, the velocity of a tachyon in the reference frame S_0 will be always the same, regardless of the direction of propagation. We will call that velocity $\beta_t c$, with $\beta_t > 1$. The existence of several types of tachyons, associated to different values of β_t could be supposed; however, all of them will have the same reference frame S_0 as support. Once called βc the velocity of S with respect to S_0 , the velocity of a tachyon with respect to a general reference frame S could be obtained from the known law of the speed composition (which is a direct consequence of Lorentz's transformations).

Finally, let us now analyse Møller's demonstration considering our alternative hypothesis. We call βc e $\beta' c$ respectively the velocities of S and S' in relation to S_0 . We also say that the first tachyon (the one with the equation of motion $x' = -u't'$ in S') has the speed $\beta_t c$ in S_0 (that is its equation of motion in S_0 will be $x = -\beta_t ct$), whereas the second tachyon (the one with equation of motion $x = w(t - t_1) + x_p$ in S) has the velocity $\bar{\beta}_t c$ in S_0 . From the law of the speed composition quoted above we obtain:

$$\frac{u'}{c} = \frac{\beta_t + \beta'}{1 + \beta' \beta_t} \quad (3)$$

and

$$\frac{w}{c} = \frac{-\bar{\beta}_t + \beta}{\beta' \bar{\beta}_t - 1}. \quad (4)$$

The speed of S' with respect to S will be equal to

$$v = \frac{\beta' - \beta}{1 - \beta' \beta_t} c. \quad (5)$$

Using (3)-(5), (2) can be written in the form $\frac{v}{c} > \frac{\frac{v}{c}(1 + \beta_t \bar{\beta}_t) + (\beta_t + \bar{\beta}_t)}{\frac{v}{c}(\beta_t + \bar{\beta}_t) + (1 + \beta_t \bar{\beta}_t)}$ equivalent to

$$\frac{(\beta_t + \bar{\beta}_t) \left[1 - \left(\frac{v}{c}\right)^2\right]}{\frac{v}{c}(\beta_t + \bar{\beta}_t) + (1 + \beta_t \bar{\beta}_t)} < 0. \quad (6)$$

The numerator of the left member in the inequality (6) proves trivially positive for every $v \in (-c, c)$. The sign of the denominator is easily obtained noting that $(v/c)(\beta_t + \bar{\beta}_t) + (1 + \beta_t \bar{\beta}_t) > 0 \Leftrightarrow (1 + \beta_t \bar{\beta}_t)/(\beta_t + \bar{\beta}_t) > -(v/c)$, inequality which is satisfied for every $v \in (-c, c)$, being $(1 + \beta_t \bar{\beta}_t)/(\beta_t + \bar{\beta}_t) > 1$ if, as in our case, $\beta_t > 1$ and $\bar{\beta}_t > 1$. Since both numerator and denominator are positive, we obtain that (6) is never satisfied, it means that,

⁵Møller reaches the thesis $t_2 < 0$, hypothesizing that $u' > c^2/v$ and $w > (u' - v)/(u'v/c^2 - 1)$ are chosen (eq. (53) in [6]), but, in my opinion, doing so it is not clear if the thesis is to be understood as always valid, i.e. regardless of the velocities of the tachyons hypothesized, subject to the adequate choice of v .

assumed our alternative hypothesis, it is not possible to determine any $v \in (-c, c)$ which makes the situation presented by Møller paradoxical. The tachyons ruled by a preferred reference frame which supports their propagation do not arise any causal paradox.

5. Local realism

Let us analyse some other extracts from the interview to Bell already mentioned [2] (pages 48-50):

Question (P. C. W. Davies & J. R. Brown): *Bell's inequality is, as I understand it, rooted in two assumptions: the first is what we might call objective reality - the reality of the external world, independent of our observations; the second is locality, or non-separability, or no faster-than-light signalling. Now, Aspect's experiment appears to indicate that one of these two has to go. Which of the two would you like to hang on to?*

Answer (J. Bell): Well, you see, I don't really know. For me it's not something where I have a solution to sell! For me it's a dilemma. I think it's a deep dilemma, and the resolution of it will not be trivial; it will require a substantial change in the way we look at things. But I would say that the cheapest resolution is something like going back to relativity as it was before Einstein, when people like Lorentz and Poincaré thought that there was an aether - a preferred reference frame - but that our measuring instruments were distorted by motion in such a way that we could not detect motion through the aether. Now, in that way you can imagine that there is a preferred frame of reference, and in this preferred frame of reference things do go faster than light. But then in other frames of reference they seem to go not only faster than light but backwards in time, that is an optical illusion.

Q: *Well, that seems a very revolutionary approach!*

A: Revolutionary or reactionary, make your choice. But that is certainly the cheapest solution. Behind the apparent Lorentz invariance of the phenomena,

there is a deeper level which is not invariant.

Q: *Of course the theory of relativity has a tremendous amount of experimental support, and it's hard to imagine that we actually go back to a pre-Einstein position without contradicting some of this experimental support. Do you think it's actually possible?*

A: Well, what is not sufficiently emphasized in textbooks, in my opinion, is that pre-Einstein position of Lorentz and Poincaré, Larmor and Fitzgerald was perfectly coherent, and is not inconsistent with relativity theory. The idea that there is an aether, and these Fitzgerald contractions and Larmor dilations occur, and that as a result the instruments do not detect motion through the aether - that is a perfectly coherent point of view.

Q: *And it was abandoned on grounds of elegance?*

A: Well, on the ground of philosophy; that what is unobservable does not exist. And also on grounds of simplicity, because Einstein found that the theory was both more elegant and simpler when we left out the idea of the aether. [...] The reason I want to go back to the idea of an aether here is because in these EPR experiments there is the suggestion that behind the scenes something is going faster than light. Now, if all Lorentz frames are equivalent, that also means that things can go backward in time.

Q: *Yes, and that is the big problem.*

A: It introduces great problems, paradoxes of causality and so on. And so it's precisely to avoid these that I want to say there is a real causal sequence which is defined in the aether. Now, the mystery is, as with Lorentz and Poincaré, that this aether does not show up at the observational level. It is as if there is some kind of conspiracy, that something is going on behind the scenes which is not allowed to appear on the scenes. And I agree that that's extremely uncomfortable.

Q: *I'm sure Einstein would turn in his grave!*

A: Absolutely. And that's very ironic, it is precisely his own theory of relativity

which creates difficulties for this interpretation of quantum theory (which is in the spirit of Einstein's unconventional view of quantum mechanics).

I would say that, from the extract quoted above, we can infer that both Bell and Davies agree on the fact that the introduction of a preferred reference frame would result in a return to the pre-relativity period. This would, in their opinion, make Einstein turn in his grave! The essence of what said above in paragraph 3 is that the thesis of the incompatibility between relativity and the existence of a preferred reference frame proves groundless. Although this thesis has an almost unanimous support in the scientific community, in my opinion, it should be completed with arguments aiming at demonstrating that relativity should be based on *PRF* rather than on the more general *PR*. I personally do not see those arguments.

There is another reason why Bell defines "extremely uncomfortable" his "cheapest resolution": Bell maintains that the aether he proposes would be non observable, like the one hypothesized by Lorentz and Poincaré for the electromagnetic phenomena. We will now turn to discuss this alleged non observability.

5.1. Eberhard

In 1989 P. H. Eberhard presented a work [15] (see also [16]) in which he develops Bell's ideas expressed above giving them a much more concrete substance than what might result from a radio interview. What, according to Bell, is "something (...) going faster than light" becomes for Eberhard a "collapse operator" which is always generated locally where (and when) a measurement is taken. The measurement is to be taken on a particle belonging to a multi-particle system in an entangled state. Then the collapse operator propagates at superluminal speed and the correlations of the quantum mechanics only exist if the measurements on the other particles are made after these have been reached by the collapse operator. Essentially, the collapse operator is the tachyon, or the tachyonic wave, which "informs" the other entangled particles (or the measurements devices involved with the other entangled particles) about the outcome of the first measurement. The other particles (or the corresponding measurements devices), following the interaction with the tachyon, will have such a state to make the measurements appear correlate according

to the predictions of the quantum mechanics. For sufficiently fast measurements the tachyon might not succeed in "informing" all the other particles in due time, consequently, there would be a decrease in the correlations foreseen by the quantum mechanics. Since the measurement events of different particles are, or can generally be, space-like, assumed the standard synchronization in every reference frame, the time order of the measurement events will not be the same in all reference frames: in some of them, some tachyons will travel "backwards in time". Because of that, Eberhard talks about Lorentz non-invariance of his model. The "correct" order of the events is established by the time sequence in the reference frame (which will become preferred for this reason) in which the collapse operator propagates isotropically, that is the reference frame where, once the clocks have been synchronized according to the standard relation, the velocity of the tachyons is the same in every direction. In other reference frames the time sequence could be reversed. What Bell calls "optical illusion" becomes in Eberhard the non-invariance for Lorentz's transformations of his own model. We will get back below to the discussion of this so-called "non-invariance". Now we will focus our attention on a crucial aspect of the model, i.e. on the *observability* of the preferred reference frame, quoting directly from the conclusions of Eberhard's work mentioned above ([15] pages 204-205):

In the model, there is a privileged space-time rest frame and a parameter V with the dimension of a velocity; V is assumed to be much larger than c . If a model of this type is correct, no violation of the prediction of quantum theory is expected if time intervals between measurements are large, i.e., larger than the time necessary to travel between the locations of these measurements at the velocity $V > c$ in the privileged rest frame. In particular, no identification of the privileged rest frame would be possible using such measurements. However, by performing multiple-measurement experiments with large distances Δx and time intervals smaller than $\Delta x/V$ between measurements, violation of quantum theory may be expected. Some identification of the privileged rest frame may become possible.

If value of V is very high, the time interval $\Delta x/v$ is so small that the multiple-measurement experiments mentioned above become impractical. As long as this is true, the model will show no observable discrepancy with quantum theory. Its impact will be primarily philosophical.

If parameter V is not too large, multiple-measurement experiments of the type described above may reveal a violation of quantum theory and of Lorentz invariance at the same time. Whenever such experiments uphold the predictions of quantum theory, they allow one to derive a lower limit for the parameter V .

Called \vec{v} the speed of the preferred reference frame with respect to the Earth and v_t the parameter called V by Eberhard above, that is the module of the speed of the tachyons in the preferred reference frame, using Lorentz's transformations, the events in the reference frame of the laboratory can be described and, for the given \vec{v} and v_t , it will be possible to evaluate the features the experimental apparatus will have to have in order to highlight the loss of the correlation predicted by the model [17, 18]. The details of the references [17, 18] show how we can trace back the measurements of \vec{v} and v_t from the observation of these losses of correlation.

As stressed above by Eberhard, whatever the features of an experimental apparatus are, the model could not be considered falsified in case no loss of correlation should be observed (only a minimum value for v_t could be fixed, which, however, will prove dependent on $|\vec{v}|$). This could make somebody consider the model not scientific as not falsifiable. Personally, I cannot agree with this opinion as it would be as considering not scientific the hypothesis of spherical Earth, or of light propagating with finite speed, in view of the experimental evidence which, within the limits of measurement uncertainties, is consistent with the hypothesis of flat Earth or of light propagating at infinite speed respectively. Mention could be made that similar remarks were moved to the Copernican theory: if the Earth moved, phenomena of parallax with the stars should be observable. Galileo's answer⁶, quoting Copernicus, was that because of the big distance

of stars from the Earth, such phenomena were unobservable with the instruments available at that time; however, in principle they could have been observed with measuring instruments endowed with better resolution, as it actually happened later: in 1839 F. W. Bessel [20] published the first measurement on the annual parallax of a star (Cygni 61). At the time Galileo's answer probably sounded weak, considering also that neither Copernicus nor Galileo were able to estimate the resolution necessary to observe those phenomena of parallax, but it was the only possible one. The situation is almost identical for the model under study: only after \vec{v} and v_t are known will it be possible to estimate the minimum sensitivity an experimental apparatus must have in order to be able to falsify the quantum mechanics. Before then, it will be possible only to carry out increasingly more sensitive experiments which will only raise the minimum value of v_t consistent with the experiments which will remain consistent with the quantum mechanics.

Anyway, independently from the opinions we may have on what should be considered "scientific", it could be useful to keep in mind that the model has already attracted the attention of some experimental groups as well as prestigious magazines. A first test [19], carried out in 2000, limited the experimental analysis only to some directions of the vector \vec{v} , problem overcome in later works [17, 18]. None of the three experiments has revealed any loss of correlation. The minimum value of v_t that can be deduced from those experiments ranges between $0.6 \cdot 10^4 c$ and $1.8 \cdot 10^5 c$ for $|\vec{v}| < 0.1 c$, depending on the value of $|\vec{v}|$. For $|\vec{v}| > 0.1 c$ the limit value is inferior to $0.6 \cdot 10^4 c$ and tends to c^+ for $|\vec{v}| \rightarrow c^-$, i.e., fixing the sensitivity of an experimental apparatus, the losses of correlation would be unobservable even if it were $v_t \gtrsim c$ if $|\vec{v}|$ were sufficiently close to c [18].

5.2. Bohm and Hiley

In 1993 D. Bohm and B. J. Hiley [21] published a book in which there is a model which follows the main aspects (and faces the same problems) of the

⁶"I wish have said that if such a variation were perceived, nothing would remain that could cast doubt upon the earth's mobility, since no counter could be found to such an event.

But even though this may not make itself visible to us, the earth's mobility is not thereby excluded, nor its immobility necessarily proved. It is possible, Copernicus declares, that the immense distance of the starry sphere makes such small phenomena unobservable. And as has already been remarked, it may be that up to the present they have not even been looked for, or, if looked for, not sought out in such a way as they need to be; that is with all necessary precision and minute accuracy." [1] (pag. 489)

model presented some years before by Eberhard. The following extract is taken from pages 292-294:

For example, one could suppose that in addition to the known types of field there was a new kind of field which would determine a space-like surface along which nonlocal effects would be propagated instantaneously. At present we can say very little about this field, but one could surmise that this space-like surface would be close to a hyperplane of constant time as determined in a certain Lorentz frame. A good candidate for such a frame could be obtained by considering at each point in space-time, the line connecting it to the presumed origin of the universe. This would determine a unique time order for the neighbourhood of that point around which one would expect isotropic properties in space. We may plausibly conjecture that this frame would be the one in which the 3K background radiation in space has an isotropic distribution.

This unique frame would not only make possible a coherent account of non-local connections, but could also be significant in other ways.

[...] This brings us to the second objection, i.e. that the absolute frame is unobservable. Certainly if we restrict ourselves to the assumptions that have been made thus far, this would be a valid criticism. However, in terms of our ontological approach it is possible to alter these assumptions in such a way that the absolute frame would be observable, while negligible changes would be produced in the domain of experiments that have been possible thus far.

[...] Let us assume then that the long range connections of distant systems are not truly nonlocal, as is implied by the quantum theory, but that they are actually carried in the preferred frame at a speed that is finite, but very much greater than that of light. For measurements made at levels of accuracy thus far available, the results will be very close to those predicted by the present quantum theory. But if we can make measurements in periods shorter than those re-

quired for the transmission of quantum connections between particles, the correlations predicted by quantum theory will vanish. In effect we would thus be explaining quantum nonlocality as an outcome of a deeper kind of non-Lorentz invariant locality. This however is relevant only in a domain beyond that in which current quantum theory is adequate.

If we consider such modifications of the theory, it then becomes possible to contemplate an experimental test that would reveal the preferred frame. In essence this test would consist of doing an EPR experiment in which the relative time of detection of the two particles was extremely accurately determined. In principle the Bell inequality would then no longer be violated for there would be no time for the disturbance of one particle to propagate to the other before the measurement was made on it. And from this it follows that the latter particle would no longer go into a corresponding state of close correlation with the first one. Of course this will require a measurement of extremely high accuracy because the speed of transmission of the quantum connection is assumed to be very much greater than that of light. It is clear that the accuracy required is far beyond that of the Aspect experiment.

In certain ways this test is reminiscent of the Michelson-Morley experiment, but it differs in the crucial respect that what is at issue here is not the measurement of the speed of light, but rather a measurement of the immensely greater speed of propagation of our assumed quantum connection of distant particles. Nevertheless as in the Michelson-Morley experiment, we would have to take into account the speed of the earth relative to the preferred frame. Thus one would have to make measurements in different directions and at different times. If one detected a change between these measurements, one would be able to demonstrate the existence of a preferred frame thus making the latter in principle observable.

However, if such changes were found, this would indicate a failure of both quantum mechanics and relativity, which

would be much more significant than the mere observation of the speed of the preferred frame. The meaning of such a result would be that we can eliminate quantum nonlocality provided that we assume a deeper level of reality in which the basic laws are neither those of quantum theory nor of relativity (which latter come out as suitable limiting cases and approximations).

The measurements “of extremely high accuracy” Bohm and Hiley speak about are clearly similar to those presented in the experiments [17–19], which we have already presented in the paragraph 5.1. We can see that Bohm and Hiley too point out that, in principle, such experiments make the preferred reference frame they propose observable.

The problem which brings together Eberhard, Bohm and Hiley is the so-called Lorentz non-invariance of the model. Undoubtedly, if we hypothesize phenomena physically *different* in a certain reference frame in respect with all the others, we can’t expect to have the same descriptions of those phenomena for each reference frame. But what is at stake here is not a descriptive issue. It is the substantial issue mentioned above, which we will now at last deal with.

5.3. Lorentz invariance

How the model is non invariant for Lorentz’s transformations is clearly explained by the authors mentioned above. For instance, Eberhard [15] says (pages 178-179):

The sequence of collapses matches the time order of the measurements in the chosen space-time rest frame. When measurements are performed at different points in space outside the light cone of one another, that time order depends on the rest frame chosen. In this sense, these formalisms are not Lorentz invariant.

As I already said above, the real issue does not concern formalisms. Actually, it concerns a formalism with a much heavier relevance than this “Lorentz invariance”. The question is: does the model challenge the theory of relativity?

We have already seen that the question is asked by Davies and Brown as soon as Bell hypothesizes a “deeper level which is not invariant”:

“Of course the theory of relativity has a tremendous amount of experimental support, and it’s hard to imagine that we actually go back to a pre-Einstein position without contradicting some of this experimental support. Do you think it’s actually possible”?

However, in my opinion, Bell’s answer is definitely weak. Basically Bell advocates the return to “pre-Einstein” positions. Bell does not answer that “the tremendous amount of experimental support” consists entirely of experiments carried out “below decks”, i.e. on isolated systems, whereas the “deeper level which is not invariant” aims at reminding us that it is not sure that a certain phenomenon has necessarily all its causes below decks. It is not necessarily true that any system is isolated. A system of particles in an entangled state might interact with a non draggable ether from the reference frame in which the system is under experimental study ⁷.

Bohm and Hiley have no doubts about the issue, either; they say clearly that, in their view, the model challenges the theory of relativity:

The most essential feature of special relativity is Lorentz invariance ([21] page 289);

However, if such changes were found [that is if the losses of correlation predicted by the model were observed], this would indicate a failure of both quantum mechanics and relativity, which would be much more significant than the mere observation of the speed of the preferred frame. The meaning of such a result would be that we can eliminate quantum nonlocality provided that we assume a deeper level of reality in which the basic laws are neither those of quantum theory nor of relativity (which latter come out as suitable limiting cases and approximations) ([21] page 294).

Eberhard sounds slightly more cautious. He does not explicitly mention the violation of the theory of relativity ([15] page 170):

⁷Were it possible to drag the ether which is assumed to be the support of the propagation of the tachyons, then the demonstrations on the causal paradoxes would prove valid. My thanks to V. Moretti for this important remark on the non draggability of the ether hypothesized by the above model, which he expressed in a public discussion on usenet.

The concept of locality used here is different from the Lorentz-invariant concepts of locality that led to the EPR paradox, because these latter concepts are incompatible with superluminal effects. I believe this rudimentary concept of locality contains all the locality requirements that could have been expected from a theory before relativity was discovered. The kind of locality aimed at here is the same kind of locality obtained at the time the Maxwell equation were written.

Indeed, that “before relativity was discovered” seems to imply that, in Eberhard’s opinion, his “rudimentary concept of locality” is in contrast with relativity and that it should be in contrast because it results in a non Lorentz invariant model.

Surely Eberhard does not support the argument strongly advocated in these pages, namely that *relativity does not require all phenomena to be necessarily referable to Lorentz invariant descriptions*.

We could wonder what would be left of relativity if we took away what, according to Bohm and Hiley, is its “most essential feature”. I think it would be left what relativity has always been. Lorentz invariant description of all phenomena with causes below decks, including electromagnetic phenomena, would be left. There would still be the fact that the rules would have a Lorentz invariant behaviour (probably since they are mainly ruled by electromagnetic forces). The same applies to the clocks, which are all synchronous to the light clock, i.e. a rule at whose ends an electromagnetic signal reflects. There would still be the fact that the *reference frames* may never have a relative speed higher than the light speed in vacuum (probably as the reference frames are “rigid” rooms, made of rules which, being mainly ruled by electromagnetic forces, will never be able to move at a speed higher than the one of the electromagnetic waves). There would still be the fact that the electromagnetic phenomena do not identify any preferred direction, that the speed of light is independent of the source motion. I cannot really see what should change of relativity if we stressed that *PR* rather than *PRF* should be considered among its foundations. The possible existence of non Lorentz invariant phenomena is predicted in *PR* which suggests looking “above decks” in such occurrence. The possible experimental observation of those non Lorentz invariant effects predicted by the model discussed above may perhaps

challenge other theories explicitly based on *PRF*, which should be revised in this case, and this price should be compared to the one paid for the renunciation of the local realism. We should, however, bear in mind that, as already seen, the predictions of the model aren’t Lorentz invariant only for markedly exceptional situations: measurements “of extremely high accuracy” (and geared to the aim) are necessary to be able to identify the non Lorentz invariant effects predicted by the model. Whether to choose or not the local realism, accepting its possible consequences, is just an option. It is not the facts which force us to give up the local realism. Neither will they be able to force us in the future, at least as long as we have results consistent with the predictions of the quantum mechanics, considering that, for adequate v_t values, any experiment consistent with the predictions of the quantum mechanics will be also consistent with the local and realist model proposed above:

It should be pointed out that, whether or not such violation of quantum theory predictions is found experimentally, the model can always be used by those physicist and engineers who prefer to work with real localizable entities rather than with the present orthodox concepts of quantum theory ([15] page 172).

Naturally, if we assume the above model we will no longer be able to claim the impossibility to send signals at superluminal speed: tachyons do send signals at superluminal speed. We could think that the tachyons are “hidden”, inaccessible to the macroscopic world (I cannot see why we should put forward such a hypothesis, though); however, in a recent interesting paper, J.-D. Bancal and al. [22] have shown that “the models based on hidden influences propagating at a finite speed $v > c$ ” allow “superluminal communication [which] does not require access to any hidden physical quantities, but only the manipulation of measurement devices”. Yet, the possibility to communicate at superluminal speed is considered forbidden by relativity because of the theorems on causal paradoxes examined above, which, as we have seen, would fall if the *PRF* weren’t assumed. We might claim that to deny the *PRF*, stating the existence of a privileged reference frame, is against the ‘spirit’ of relativity, as it is to claim the possibility of superluminal communications. N. Gisin recently wrote [23]:

Everything looks as if the two parties somehow communicate behind the scene [2]; hence, since they can't communicate at a speed equal or lower than the speed of light, let's assume they do so at a speed faster than light. Such an assumption doesn't respect the spirit of Einstein relativity, [...]. To define faster than light hidden communication requires a universal privileged reference frame in which this faster than light speed is defined. Again, such a universal privileged frame is not in the spirit of relativity, but also clearly not in contradiction: for example the reference frame in which the cosmic microwave background radiation is isotropic defines such a privileged frame.

Although it seems to me that the approval of the scientific community on this matter is vast, I take the liberty of disagreeing. In my view the 'spirit' of relativity is elsewhere⁸. That is why I claim that the proposed model does not modify in any way the relativity: neither its substance, nor its form (the *PR* is among its postulates, not the *PRF*) nor even its spirit.

5.4. Causal order

In my opinion, the model under study poses a question which neither Eberhard nor Bohm and Hiley have asked. A crucial question would stand out clearly, at least in my view, were we to accept the model. I will sum up the issue already presented in other words in [29].

⁸In the book published by Schlipp in occasion of Einstein's seventieth birthday, Reichenbach says: "The logical basis of the theory of relativity is the discovery that many statements, which were regarded as capable of demonstrable truth or falsity, are mere definitions" ([24] p. 293). Among these "mere definitions" a relevant role is given to synchronization. Einstein answered: "Now I come to the theme of the relation of the theory of relativity to philosophy. Here it is Reichenbach's piece of work which, by the precision of deductions and by the sharpness of his assertions, irresistibly invites me a brief commentary. [...] To the question: Do you consider true what Reichenbach has here asserted, I can answer only with Pilate's famous question: "What is truth"?" ([25] p.676). It is also noteworthy what Einstein wrote in 1928 in the review [26] to the book [27] Reichenbach published in that year: "special care has been taken to ferret out clearly what in the relativistic definition of simultaneity is a logically arbitrary decree and what in it is a hypothesis, i.e., an assumption about the constitution of nature." (quoted in [28] pag.181).

The model deprives the light cone of the meaning commonly given. It is not possible any more to claim that only events inside the light cone are causally linkable. Besides, once the existence of events causally linked outside the light cone has been admitted, we have the immediate consequence that the time order induced by the standard synchronization is not always consistent with the causal order (this is exactly the Lorentz non invariance of the model). For instance, let us assume we have a system of two particles, *A* and *B*, in an entangled state, which is submitted to the measurements m_A on *A* and m_B on *B*, in the reference frame *R* of the laboratory. The two measurements are space-like and, given the standard synchronization of the fixed clocks in the laboratory, the instants shown on the clocks fixed where (and when) the measurements are taken are t_A and t_B respectively. If the two measurements are correlated according to what predicted by the quantum mechanics, the model predicts that the two measurements are causally linked: one measurement is concause to the other. The measurements are causally ordered: one is "the first" and from the point where the first measurement is taken leaves the tachyon which will be received by the second particle before the measurement is taken on it. The point is that the causal order does not necessarily correspond to the time order resulting from the standard synchronization: m_A might be the first measurement even if we had $t_A > t_B$. Such an occurrence does not create any bewilderment once the conventional character of the standard synchronization (as well as of any other synchronization) has been clarified [30]; obviously, however, the model must provide a criterion according to which we could establish in every circumstance *how to understand* which of the two measurements is the first. In my opinion, when providing this criterion, both Eberhard and Bohm and Hiley dodge the crucial issue expressed above. The model hypothesizes the existence of a preferred reference frame; through Lorentz's transformations we can identify the instants t'_A and t'_B shown on the clocks fixed in the preferred reference frame where (and when) the two measurements are taken (the clocks of the preferred reference frames, too, are meant to be synchronized according to a standard relation). The "first" measurement will be the one associated to the instant t' minor, i.e. if $t'_A < t'_B$ then the first measurement is m_A . Essentially both Eberhard and Bohm and Hiley take the causal order back to the time order resulting from the standard

synchronization in the preferred reference frame.

Finally let us analyse the key issue which is, in my opinion, overlooked. I agree that the model must give a criterion to establish, *in principle*, the causal order of the events and that the criterion given by Eberhard, Bohm and Hiley surely satisfies this condition; however, let us imagine the experimenter in his laboratory: he takes the measurements m_A and m_B , sees that the outcomes are correlated and, accepting the model under study, infers that one has been the concause of the other. The experimenter does not know the parameter \vec{v} , that is he does not know at what speed or towards which direction the preferred reference frame is moving with respect to the Earth. Consequently he cannot know the instants t'_A and t'_B . Must the experimenter then give up the possibility to know which of the two measurements was the “first”? Can we believe that something so important as the causal order is not “written” inside some physical quantity accessible from every reference R and nature has decided to write it only inside some instruments we may or may not decide to use to describe the events (like the clocks fixed in the preferred reference frame)? If nobody had put clocks in the preferred reference frame, would the causal order have been left “written” only in a possibility practicable in principle (and that nobody has practised)?

These are the questions that, in my view, Eberhard, Bohm and Hiley fail to consider in giving their criterion to establish the causal order of the events.

I can understand the criticism that could be moved to such questions, downgrading them to metaphysics and comparing them to wondering how many angels are able to sit on the point of a needle⁹, but I believe that in science there is not a definite criterion which can establish which questions should be considered ridiculous. Surely what matters in the end is what is “produced”, the experimental effects that are expected following our considerations. Besides, what the experiments say obviously matters. But the reasons why we make some considerations, the reasons why we consider some things credible or incredible can be varied.

⁹From the letter sent by W. Pauli to M. Born on 31 March, 1954:

“As O. Stern said recently, one should no more rack one’s brain about the problem of whether something one cannot know anything about exists all the same, than about the ancient question of how many angels are able to sit on the point of a needle. But it seems to me that the Einstein’s questions are ultimately always of this kind.” [31] (pag. 223)

I do not think questions such as the ones quoted above must necessarily be considered important, I do believe, however, that they can not be considered ridiculous precisely as we should not consider ridiculous the question: “Is it possible that nature does not work in a realist and local way?” A realist might consider it ridiculous to believe that nature works in a non local way, nevertheless, in my opinion, he must anyway take into account theories based on assumptions which appear ridiculous to him and must then evaluate those theories on the basis of their predictive ability. Similarly, I think an orthodox should consider questions ridiculous for him (metaphysical stuff) and evaluate them on the basis of what they “produce”. The “product” I intend to give in this chapter will be an alternative criterion to understand how to identify the causal direction (criterion I consider credible since the one given by Eberhard, Bohm and Hiley seems to me not very credible).

Questions similar to those examined above could be asked also about subluminal physics. A billiards ball bounces between the cushions. Without fixing clocks on each cushion and without synchronizing them according to a definite criterion¹⁰, would we be unable to know which of the two bounces was the concause of the other, that is, which is the first and which is the second? Is it possible that something so important as the causal order is not *written inside the system* under study (i.e: the billiards, with its cushions and the ball) and it is written only inside measurement instruments (the clocks fixed on the cushions) which we may even decide not to use and which also have been synchronized according to a convention we have chosen ourselves? Without using the clocks, is it impossible to know which is the cause and which the effect? Naturally some measurements will have to be taken in order to obtain some physically meaningful information about the system, some measurement instruments will have to be used, but the clocks are meters of time intervals, they are not meters of the causal direction. The criterion followed to establish the causal direction using clocks is very complicate! We call *CR* such a criterion, which is the following:

the clock fixed on a cushion measures the time interval t_1 from the moment when it was set at the instant 0 to the moment when it sees the ball boun-

¹⁰Actually it can be easily demonstrated that infinite synchronizations exist leading to a time order consistent with the causal order.

cing on its cushion, the clock fixed on the other cushion measures a time interval t_2 from the moment when it was set at the instant 0 to the moment when it sees the ball bouncing on its cushion; the criterion followed to set the clocks at 0 is the standard synchronization. On the basis of the comparison between the two time intervals t_1 and t_2 it is established which bounce was the first, that is which of the two caused the other (in the hypothesis that the two events are causally linked).

It is only our unconscious habit to consider time as it had always stayed in the apriori Olympus which makes the criterion just examined sound trivial to us. It seems obvious to us that the cause must take place before the effect and it seems obvious to us that we must give that “before” a meaning linked to what is shown on the clocks: the cause must be the one taking place where it is fixed the clock showing an instant less than the one shown on the clock fixed where the effect will take place. However, getting time down from the apriori Olympus, relativity teaches us that we are giving that “before” a not so obvious meaning. A meaning which derives from a complicate criterion with conventional aspects as well! I also think we should remark the consideration that it may seem strange that nature has written the causal direction inside such a complicate criterion.

We have already seen that the criterion *CR* does not work if the two events are connected causally through the information exchange occurred at superluminal speed and that this information exchange would not create causal paradoxes if we decided *not* to assume *PRF* as one of the foundations of physics. Once *PRF* has been assumed instead, the impossibility of superluminal signals is demonstrated (or causal paradoxes would appear) and it is also possible to demonstrate that the criterion *CR* works regardless of the reference frame where the events are described and that, with adequate modifications, it also works independently of the chosen synchronization; this leads to call, improperly in my opinion, the derived structure “causal structure”. Essentially, despite the conventionality of the standard synchronization, it is demonstrated that, assumed the *PRF*, the time order induced by the standard synchronization is always consistent with the causal order. At this point the criterion *CR* has the right credentials and if somebody thinks it is a bit too complicate to be considered the “true” criterion in which nature has written the causal direction, we can surely reply that this is his own

problem. When I say that the criterion *CR* has the right credentials, I mean the following: being in the reference frame *R*, in order to be able to identify the causal order, it will be necessary to act in a way that might be complicate for somebody, but it is always only a question of measurements taken inside *R*. And they are measurements connected with the system under study. In our example, they are measurements related to the billiards, the ball; measurements employing clocks fixed in *R*, rules fixed in *R*, possibly luminous signals (to synchronize the clocks, should we decide to synchronize them through a luminous signal). The criterion *CR* does not require anything “odd”. For instance it does not require us to know where the billiards comes from, when it was brought to the bar where we are carrying out the experiment, who the manufacturer is ... It only requires measurements to be taken on the billiards, using instruments available in *R*.

The situation *changes radically* when we abandon *PRF* and, as both Eberhard and Bohm and Hiley do, hypothesize the existence of a preferred reference frame for the propagation of superluminal signals. We are now still in the reference frame *R* and deal with our two-particle system in an entangled state. We now examine the measurements m_A and m_B on the system under study, we possibly determine also the time intervals t_A and t_B shown on the clocks fixed in the place where the measurements are taken (each of the two intervals starts when the clock is set at 0 and finishes when the corresponding measurement is taken), we determine the distance between the points where the measurements are taken; in short, we take all the measurements we might consider useful in order to know which of m_A and m_B was the cause and which the effect, but we realize that the criterion established by both Eberhard and Bohm and Hiley require us also to take definitely odd measurements! That criterion says that we first have to determine \vec{v} (i.e. the speed of the preferred reference frame with respect to *R*) so that, through Lorentz’s transformations, we can then determine the instants t'_A and t'_B comparing which we could finally find out which between m_A and m_B was the cause. It is true that a criterion, to be such, must explain how to solve the problem in principle (and it is true that in principle the problem can be solved in the way proposed by Eberhard, Bohm and Hiley), however, this criterion, in my opinion, *has not* the right credentials. It would be as if, getting back to the subluminal world and

to our example of the billiards, the criterion CR , rather than requiring only measurements taken in R on the system under study, would also require us to know, for example, the manufacturer of the billiards or the place where it was produced. That is why, in my opinion, the model presented by Eberhard and Bohm and Hiley *requires* the research of a *credible criterion*, on the base of which the causal direction can be established.

The criterion proposed in [29] can be summarized as follows, at least in its fundamental aspects:

any time two events are connected causally there is *always* a *messenger* which propagates from the point where the cause occurs to the point where the effect takes place. The messenger could be a particle or, more generally, a signal of any type, subluminal, luminous or superluminal. A momentum is *always* associated to the messenger. The direction of the momentum vector of the messenger is always from the cause to the effect. Given two events causally connected, in order to identify which is the cause and which the effect, we measure the momentum of the messenger.

5.5. Criterion of causality and time ordering.

In a given reference frame R we consider two events associated to the quadrivectors (ct_1, \vec{x}_1) and (ct_2, \vec{x}_2) respectively. It will be possible to associate to the two events the quadrivector $\Delta x_\mu \equiv (\Delta ct = ct_2 - ct_1, \Delta \vec{x} = \vec{x}_2 - \vec{x}_1)$, which, like all quadrivectors, enables us to define the corresponding invariant:

$$(\Delta x_\mu \cdot \Delta x_\mu) \equiv (\Delta ct)^2 - (\Delta \vec{x})^2.$$

It can be easily shown that $(\Delta x_\mu \cdot \Delta x_\mu) > 0 \Leftrightarrow (\Delta \vec{x})^2 / (\Delta ct)^2 < 1$, that is the sign of the invariant $(\Delta x_\mu \cdot \Delta x_\mu)$ can discriminate the pairs of events connectible through subluminal signals from those not connectible through subluminal signals: in the former case the invariant is positive, in the latter it is negative. Let's now limit the analysis to the case $(\Delta x_\mu \cdot \Delta x_\mu) > 0$ and hypothesize that the two events are linked by a causal relation which could be easily identified. For instance the event (ct_1, \vec{x}_1) corresponds to the creation of a certain particle (cause) and the event (ct_2, \vec{x}_2) corresponds to the detection of the particle (effect). In accordance with how it has been defined, the vector $\Delta \vec{x}$ heads from the cause to the effect. However, we could hypothesize a case in which it is not known if the particle has been generated in \vec{x}_1 or in \vec{x}_2

(i.e. if $\Delta \vec{x}$ must be defined as $\vec{x}_2 - \vec{x}_1$ or $\vec{x}_1 - \vec{x}_2$), in such a case we could turn to the reading of the instants t_1 and t_2 to identify the causal direction. It can be easily shown that, if the events can be connected through particles in subluminal motion, the cause must be associated to the event with the lower instant. Indeed, assuming that the particle is generated in \vec{x}_1 and detected in \vec{x}_2 , under the standard synchronization, we will have that the clock in \vec{x}_2 will indicate the instant $\bar{t} = t_1 + |\Delta \vec{x}|/c$ exactly when it is reached by a light beam leaving \vec{x}_1 simultaneously with the creation of the particle. Moving subluminally, the particle will reach the point \vec{x}_2 after the light beam, that is when the clock set in that point will indicate $t_2 > \bar{t}$. Having synchronized the clocks fixing $\bar{t} > t_1$, it will follow $t_2 > t_1$. We could, however, identify the causal direction without turning to the reading of the instants t_1 and t_2 . An energy-momentum quadrivector is associated to the particle and we could define it as

$$p_\mu = (E, \vec{p}) \equiv \sqrt{\frac{(p_\mu \cdot p_\mu)}{(\Delta x_\mu \cdot \Delta x_\mu)}} \Delta x_\mu. \quad (7)$$

The (7) is to be understood as follows: p_μ is proportional to Δx_μ , according to a positive constant which will necessarily correspond to the root of the ratio between the invariants $(p_\mu \cdot p_\mu)$ and $(\Delta x_\mu \cdot \Delta x_\mu)$. Particularly from (7) it follows that the vector momentum associated to the particle will be defined as

$$\vec{p} \equiv \sqrt{\frac{(p_\mu \cdot p_\mu)}{(\Delta x_\mu \cdot \Delta x_\mu)}} \Delta \vec{x},$$

that is \vec{p} is by definition concordant to the causal direction (i.e. its direction is the same as the direction of $\Delta \vec{x}$). This means that in case we did not know if the cause was in \vec{x}_1 or in \vec{x}_2 (that is if the correct definition of $\Delta \vec{x}$ was $\vec{x}_2 - \vec{x}_1$ or $\vec{x}_1 - \vec{x}_2$) we could resort to measuring the vector \vec{p} (particularly to measuring the direction of that vector) to identify the causal direction.

We can observe, as noticed above, that the fact that the sign of $\Delta ct = ct_2 - ct_1$ indicates the causal direction results from the definition of Δct (e.g. from the assumed synchronization) as well as from the fact that the chosen events are connectible through subluminal motion, namely that $(\Delta x_\mu \cdot \Delta x_\mu) > 0$. Therefore we should not be surprised if for $(\Delta x_\mu \cdot \Delta x_\mu) < 0$, that is, for pairs of events connectible through superluminal motion,

the sign of Δct induced by the standard synchronization could fail to indicate the causal direction. Indeed, assuming that in \vec{x}_1 a tachyon is generated, which will later be detected in \vec{x}_2 , even if we had the “right” sign for $\Delta ct = ct_2 - ct_1$ (i.e. was $\Delta ct > 0$) in R , we could always find appropriate reference frames R' , moving with velocity $\vec{\beta}c$ with respect to R , for which will be $\Delta ct' = ct'_2 - ct'_1 < 0$. Indeed Lorentz transformations give

$$\Delta ct' = \frac{1}{\sqrt{1 - \vec{\beta}^2}} (\Delta ct - \vec{\beta} \cdot \Delta \vec{x})$$

and being $(\Delta x_\mu \cdot \Delta x_\mu) < 0 \Leftrightarrow |\Delta \vec{x} / \Delta ct| > 1$ it will be always possible to determine appropriate $\vec{\beta}$, with $|\vec{\beta}| < 1$, so to make $(\Delta ct - \vec{\beta} \cdot \Delta \vec{x}) < 0$. As often stated above, the time ordering induced by the standard synchronization can not be always (i.e. for every reference frame) concordant with the causal ordering. In my opinion, we should always accept without doubt the fact that the causal ordering of events must be totally independent of the reference frame chosen to describe the same events. Nevertheless vast literature exists on the so-called “principle of reinterpretation” [32] according to which cause and effect could reverse their roles depending on the reference frame in which the events are described. In our example, according to the reference frame R the tachyon is emitted in \vec{x}_1 and detected in \vec{x}_2 , according to the reference frame R' the same tachyon would be emitted in \vec{x}'_2 and detected in \vec{x}'_1 . This in order to “preserve” the time ordering, to admit the possibility of superluminal signals while continuing to claim that all types of signals are generated “temporally before” being detected; meaning with temporally before the time ordering induced by the standard synchronization. Here we strongly object to this presentation and simply accept the fact that the time ordering is not a criterion which allows to identify the causal direction in the case of superluminal signals.

We have seen above that for subluminal motions we could identify the causal direction even without resorting to the time ordering: we could simply measure the momentum \vec{p} of the particle propagating from the point where the cause occurs to the point where the effect takes place. According to the local realism we can hypothesize that there is *always*, for every pair of events linked by a causal relation, a “messenger” propagating from the point where the cause occurs to the point where the effect

occurs. We hypothesize that for superluminal signals it is possible to identify an energy-momentum quadrivector in the same way as for the subluminal particles, namely according to¹¹ (7). The criterion proposed to determine the causal direction of events causally connected is to measure the direction of the momentum vector of the messenger propagating from the point where the cause occurs to the point where the effect takes place.

6. Conclusions

The realist and local model proposed by Eberhard in 1989 and presented again by Bohm and Hiley in 1993 is perfectly consistent with relativity once stressed that this must be based on the principle of relativity as originally expressed by Galileo. All known experimental evidence are consistent with the model.

As it is known, Einstein’s corpse was cremated and the ashes dispersed in an unknown place. Thus his grave does not exist; however, quoting the figure of speech used above by Davies, Brown and Bell, we could say that observing the scientific community pay scarce attention to a realist and local model, consistent with all known experimental evidence and with relativity, this would definitely make somebody turn in his grave: Einstein.

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¹¹ We can notice that from (7), it appears that $(\Delta x_\mu \cdot \Delta x_\mu)$ and $(p_\mu \cdot p_\mu)$ have the same sign so the radicand in (7) will be always defined. Consequently for the superluminal signals also the invariant $(p_\mu \cdot p_\mu)$ will be negative.

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